Figure 5 is a diagram of the composite curve of temperature from 1872. The deviations of the maximum and minimum phases on either side of the curve of best fit are, as stated above, partly due to the 11-year variation and partly due to other causes, among which are certain uneliminated short-period and accidental features of the data.

IX. CONCLUSION

The object of the investigation has been, as previously stated, the determination of the epochs of maxima and minima of the short period for restricted areas, and the systematic variations in its length. This has been accomplished satisfactorily by consideration of data for temperature and pressure in Europe and the United States only, where long homogeneous records are available.

The variations of rainfall have been given only secondary and partial consideration, since examination of data from the relatively few stations from which the epochs of temperature and pressure were derived have led to indefinite results for rainfall, owing to the local and fortuitous features of this element. Records from a large number of stations must be averaged to yield satisfactory results.

Large regions of the globe including Africa, Asia, and most of the Southern Hemisphere have been left for future work. The regions adjacent to the eastern Indian Ocean, including Australia, have been studied rather intensively so that the epochs for this section, well represented by Batavia, can be said to be definitively established.

The amplitude of the period has not been studied to any extent, since its exact determination is a complicated process which must be left for future development. From inspection of the curves certain qualitative conclusions may be drawn. It is probable that the amplitude varies directly with the wave length, as illustrated by the amplitude of the temperature wave at Portland (Section V); also that the amplitude is smaller than that of variations of longer period, as the 7-year period.

Regarding the correlation found to exist between solar and meteorological data, the paper presents simply the findings of the investigation without adequate physical reason for such relationship, which appears to be of a causal nature. In this, as in many other cases, theory must wait upon observation and there is much to be done before a satisfactory basis for induction can be said to exist.

DISCUSSION

By CHARLES F. MARVIN

Convinced of the great importance of serious and searching investigations of solar and terrestrial correlations and of the laws of sequence of weather conditions, the writer has encouraged and supported such work at the central office of the Weather Bureau to the fullest extent permitted by the limited personnel and funds available.

While the present paper represents the results of only a recent investigation, nevertheless these matters have been a subject of unofficial work and study by Mr. Clough for fully 20 years, all of which constitutes a substantial foundation and background for the present contribution.

Unwilling to publish in the MONTHLY WEATHER REVIEW a paper on the illusive question of periodicities by a member of our staff which could not command the approval, at least in general and tentative terms, of myself and others, this paper has received more than the usual critical examination, both by the committee on scientific papers and myself, with the result that it is believed Mr. Clough supports his findings by a substantial array of proofs derived directly from (1) a large and acceptable body of meteorological data, (2) reference to a series of seemingly incontrovertible statistical criteria, and (3) solar and terrestrial correlation of material significance. The study has been purely an inductive one and wholly lacks suggestions as to the physical causation of the findings. The task of the critic, therefore, is, of course, to successfully refute the argument, to interpret the evidence in other terms, and to show that the findings as a whole or in part are not the facts they are represented to be. If disproofs are not forthcoming in adequate form, we must conclude that Mr. Clough's views constitute an important contribution to the laws of sequence of weather conditions.

Unfortunately, readers but partly acquainted with the literature and the technique of the subject of weather periodicities will probably find it difficult to pick out the essence of Mr. Clough's findings from the mass of details which he has deemed it necessary to present in order to meet and forestall probable criticism and exceptions.

His claims are:

(1) The sun-spot cycle is a prototype of other solar and terrestrial periodicities, many of them being obscure and unknown.

(2) The seemingly erratic and irregular changes in lengths of such of these periods as some recognize, are due only in part to accidental causes, errors of determination, etc. In addition, some as yet unknown but dominant physical influences cause the lengths to change in a systematic manner.

(3) That there is a rhythmic response and a corresponding correlation between solar and terrestrial periodic phenomena.

Assuming that we have fairly stated Mr. Clough's major findings, let us examine critically some of the evidence and proofs thereof.

Raw material and smoothing formula.—With very few exceptions, the statistical data employed were in the form of 6-month means smoothed by the formula $(a+b)\div 2$, which gives a result exactly the same as 12-month means taken at 6-month intervals. While the amount of the smoothing in this case is very slight, nevertheless it must be recognized that the application of any smoothing formula to a sequence of numbers *tends to create consecutive correlation where none previously existed.* Furthermore, where obscure correlation of a periodic character already exists the smoothing tends to reduce or efface the amplitude and to shift or alter in a more or less unassignable way important phase relations.

Suppose a, b, c, d, e, f, etc., represent an irregular sequence of perfectly unrelated numbers. Let this sequence be smoothed by any such formula as, say $(a+2b+c)\div 4$, etc. There is at once created an implied relation of an obscure character between each derived value and those immediately contiguous thereto.

These considerations apply to all such results as are represented by the curves in Figures 1 and 2 of Mr. Clough's paper. However, the practice of using smoothing formulæ in cases of this character is all but universal, and Mr. Clough's use of them seems to be as fair and conscientious as that of any other investigator. Accordingly, we find no adequate ground for the rejection as a whole of the dates of maximum and minimum phases

which he has conscientiously picked out and listed in his Table 1. About the only fair criticism which can be made of these results is that they may be accepted on a qualitative basis, but can hardly be regarded as quantitatively exact. In the first place, the method leads to practically no exact information at all as to the amplitude of the wave forms, and the time units are too large to give sufficiently accurate values.

Time units too large.—Phase values at intervals of 6 months give, on the average, only 4 to 5 points per individual wave form. This would be quite sufficient for a well-defined symmetrical wave of uniform length and amplitude, but such an interval is quite too great to develop accurately the characteristics of this obscure, complex, and widely fluctuating 28-month period, as Mr. Clough describes it. Even his smaller estimated half-unit intervals help but little, as will more fully appear in what follows.

Changes in length of period are systematic.—Many investigators have recognized that the sun-spot cycle exhibits large and seemingly erratic changes in its length. For many years Mr. Clough has insisted that these and like changes in other periods are systematic. It is now proposed to critically analyze this claim for just a single portion of the data given in his Table 1, namely, the 61 intervals or wave lengths for temperatures in the United States, using the dates for minima only. These values are found to be fairly similar to the composite data shown in Mr. Clough's Figure 3 for the maxima and minima combined. The shortest length of period is 1.50 years, the longest 3.25 years.

Referring to Figure 6 it is easy to see how the entire 61 values in Table 1 can be represented by the simple numbers 1, 2, 3, ... 8, which for further purposes have been inscribed on 61 small cardboard disks, as suggested in the illustration. The small numbers beneath indicate the frequency of each disk. The marked irregularity, skewness, etc., of the frequency distribution of these 61 numbers is clearly apparent in the little diagram at the left, with its Gaussian curve of best fit. The following constants apply to the distribution:

Average length =4.31 units =2.33 years.

Mean deviation =md =1.33 units =3.32 years.

Standard deviation =σ = $\pm$1.71 units = $\pm$4.28 years.

Cornu's ratio, $\sigma \div$ md =1.29, should be 1.25 for Gaussian numbers.

The frequency of the length 5 (2.5 years) is much too high, compared with that of the other lengths. Coarseness of time unit must have a good deal to do with such an irregularity, but random sampling and the smallness of the total number of cases (61) must not be overlooked. Since we are supposed to be dealing with a quasi-periodic function we should notice that the frequency distribution of a table of natural sines is diametrically opposite to that of Gaussian numbers. That is, sines show a U-shaped distribution in which large departures from the mean are most numerous and small departures infrequent, with a Cornu ratio about 1.11. When its value is nearly 1.25 (or the reciprocal thereof, 0.80) Mr. Clough cites and uses the Cornu ratio as an index of conformity to Gaussian numbers. He regards the ratio 1.29 in the present example as indicating that the 61 numbers are normally distributed. Our own inferences are that the Cornu ratio alone is a very poor index of conformity or nonconformity to Gaussian numbers. Definite information of this character can be obtained only by setting up the actual distribution and plotting the Gaussian curve of best fit.

In the present example, the abnormally high frequencies 5 and 4, of the extreme lengths 1 and 8, is in itself evidence of ill-defined periodicity and as shown by a slight conformity to a sine distribution masked by disturbing conditions. The distribution of the 61 numbers in itself, therefore, carries a slight support of Mr. Clough's claim, but we have not examined other cases sufficiently to know whether this feature is general or not.

It is very important to recognize at this point that whatever physical influences have conspired to yield

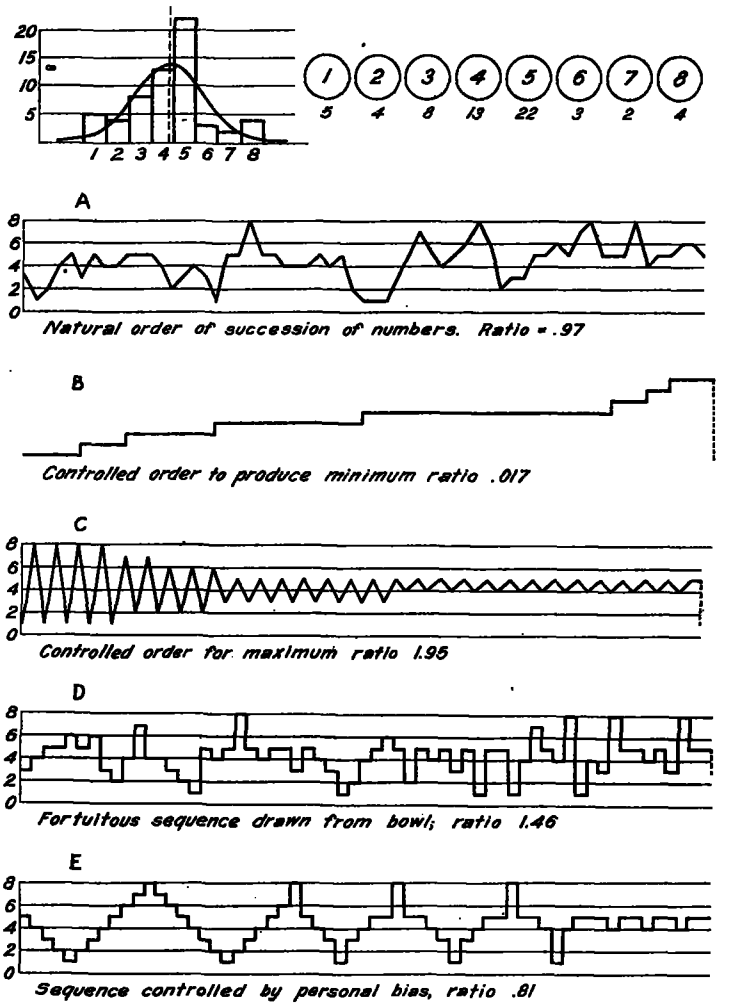

FIG. 6.—Diagram showing the frequency distribution of 61 numbered disks which represent the varying lengths of the 28-month period of temperature in the United States as measured from minimum to minimum. The natural order of succession of these lengths (graph A) is regarded as controlled because of the smallness of the Goutereau ratio, 0.97, whereas the ratio for a fortuitous sequence (graph D), by theory should be nearly 1.41. Graphs B and C represent sequences designed by personal intention to produce the minimum and maximum values of the ratio. Graph E is a sequence of the same numbers designed to have a periodic character resulting in a small value of the ratio, 0.81.

these 61 numbers possessing the statistical characteristics shown by the frequency diagram and numerical data just given, *the order of succession* has nothing to do with any of the above values. The numbers in their natural order satisfy the above distribution, but it is well known from the arithmetic of permutations that there are all but an infinite number of other orders of succession than the natural one, all of which likewise satisfy the same distribution. Mr. Clough maintains that the length of this period varies systematically; that is, there is something which makes the natural order of succession different from any other order that just happens by chance, in which case the variations in length must be regarded

as accidental. One of his proofs of this claim is that a certain statistical index, which may appropriately be called Goutereau's ratio,[28] has the small value of 0.97 for the numbers in their natural order of succession, whereas theory shows this ratio should have the value $\sqrt{2}$ or 1.41 for a wholly unrelated sequence of the numbers.

This is a very important point, and in order that each reader may understand and appreciate the full force of this argument we shall carefully define what Goutereau's ratio is. Every one knows that the algebraic sum of the *departures* of any group of numbers whatever from the *mean* or *average* is zero. The sum of all the departures *regardless of sign* divided by the number gives the *average departure* or *mean deviation*, a statistical index which is, like the mean itself, wholly independent of the order of succession.

Now to base any claim or argument upon a particular order of succession of a given set of numbers we must have a suitable measure or statistical index, depending upon the order of succession alone. Happily this index is the *mean variation*, *v*. In any irregular sequence of numbers a, b, c, d,....x, form the consecutive differences b–a, c–b, d–c..... a–x.[29]

As in the case of the departures from the mean, we find that the *algebraic* sum of all the variate differences, if taken in a ring, is zero. The sum *regardless of sign* divided by the number of values is the *mean variation*, which is the index for the order of succession we desire, and resembles and may be compared with the unchanging index afforded by the *mean deviation* of the same numbers regardless of order of succession.

Goutereau, with the aid of Maillet, has shown that the ratio $v \div md = 1.41$ for Gaussian numbers in fortuitous order of succession. Thus we have a very valuable index which closely relates the order of succession to the mean deviation of the same numbers.

With this explanation of Goutereau's ratio, each reader should easily appreciate the significance of what follows.

Referring again to Figure 6 the natural order of the 61 numbers with its ratio of 0.97 appears at A. From arithmetic we find there are more than 21×10^{41} possible different sequences. Two of these, arbitrarily set up to show the minimum and maximum Goutereau ratios, are shown at B and C. The 61 numbered disks were placed in a small bowl and from 10 separate drawings 10 values of the Goutereau ratio were computed, yielding an average value of 1.36. The lowest value was 1.24, the highest 1.51. One sample drawing is shown at D. The smallness of the value 1.36 as compared with 1.41 may or may not be significant. It seems that 10 drawings should be expected to give an average value of the ratio very close to 1.41, but it is probable in the present case that the original 61 numbers depart sufficiently from a Gaussian distribution to explain the relative smallness of the average of the 10 drawings.

The graph at E shows one result of personal control and bias in intentionally setting up a periodic sequence as far as the numbers themselves permit, giving the low ratio 0.81.

It seems utterly improbable that any fortuitous sequence could give the natural order of succession with its low ratio 0.97 any more than chance drawings could produce the selected orders of succession B and C, giving respectively the minimum and the maximum values of the ratio, or even the sequence E with its ratio 0.81.

The claim that the natural sequence of the numbers is a controlled sequence and not an accidental one can not be brushed aside either because a physical explanation of the control is wanting or without refuting or offering some better interpretation of the evidence than the foregoing, which deals with but a fragment of the whole body of data, correlations, etc., submitted by Mr Clough.

To conclude this brief note, we must recognize that in its present form we are undoubtedly dealing with a very complex feature of periodicity, probably made up of two or several elemental forms. It is well known that two periods differing slightly in length, one with large amplitude and the other with small, yield a composite period which appears to change its length systematically. Quite an extended examination on my part of data by the Fourier analysis gives little hope of explaining the observed facts in that way. A large number of elemental periods always appear to be necessary to even approximately represent the observations. Nevertheless, the subject has by no means been adequately investigated and is entitled to the serious attention of students and critics alike.

[28] See footnote no. 14, ante.

[29] By including the last term (a–x) we literally assume that the numbers constitute a closed system as if in a ring. This also causes the number of variate differences to be the same as the number of values, also the same as the number of departures from the mean. If the difference a–x is *not* included, the number of variate differences will be n–1 for n observations, and other technical inconsistencies arise which become inconsequential, however, when n is very large, which, on the other hand, is seldom the case. Therefore, we prefer to analyze all these cases as if the observations constituted a closed ring or system.

VAN BEMMELEN ON THE INTRATROPICAL PART OF THE GENERAL CIRCULATION OF THE ATMOSPHERE

551.513 : 551.55

By B. M. VARNEY

(Weather Bureau, Washington, September 25, 1924)

Dr. W. van Bemmelen in a recent paper in the Meteorologische Zeitschrift[1] makes one of the most import contributions to the study of the general circulation of the atmosphere that has appeared in recent years. During the years 1909–1917 at Batavia a total of 869 pilot-balloon flights were carried out, most of them being observed by double theodolite.[2] Doctor van Bemmelen has previously discussed the Batavia observations and offered tentative explanations for the phenomena observed.[3] His paper here summarized extends and somewhat revises his former views. Correlated with the results of the balloon flights as the basis for his discussion is evidence as to the intratropical circulation deduced from cirrus movements in the region.

Method of summarizing the data.—The author takes care to point out that the paucity of observations from the higher levels renders interpretation of the conditions there somewhat doubtful, and then states his method as follows:

> For each level and each month the north and east components of all observed wind vectors were combined and the means computed. With the aid of these means two isopleth diagrams were constructed, one for the north component and the other for the east, using altitudes as ordinates and months as abscissæ and from them the mean directions and velocities were computed. Such procedure is obviously justified where constancy of direction of the air streams is a notable feature.

For the purposes of this note, the writer has translated the results of the above procedure from van Bem-

[1] Volume 41, 1924, no. 5, pp. 133–141.

[2] Results of the observations are dealt with in Transaction Nos. 1 and 6 of the observatory.

[3] Proc. R. Acad. of Sciences, Amsterdam, Apr. 26, 1918. A paper by W. van Bemmelen and J. Boerema, "Horizontal oscillation of the free atmosphere up to 10 km. at Batavia," published in Proc. K. Akad. Amsterdam, 1917, vol. 20, pp. 119–135, was abstracted in Science Abstracts, Sec. A, Nov. 30, 1917, No. 1235, this abstract being reprinted in the MONTHLY WEATHER REVIEW, January, 1918, 46: 22. See also van Bemmelen, W., "The antitrades," MONTHLY WEATHER REVIEW, 1922, 50: 90–91, reprinted from Nature, Feb. 9, 1922, pp. 172–173, and brief discussion of this paper by Shaw, W. N. in same REVIEW, p. 92, reprinted from Nature, Feb. 16, 1922.